



Some ideas on enhancing research productivity

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ABSTRACT

Through literature sources, personal trial and error experiences, and observations of students and other colleagues, the author has developed a collection of ideas regarding enhancing research productivity. These are laid out in this article, including a number of examples and relevant quotes from the literature. The paper should be of particular relevance to Ph.D. students and others at early stages of their research careers.

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1. Introduction

The ideas in this paper come from my extensive personal experience in carrying out research, including collaboration with many different colleagues and the supervision of a large number of students. Other sources include the observation of the research behaviour of these and other individuals, as well as the examination of portions of the rather diverse literature related to research productivity.

The focus of the paper is the individual researcher although some points will be raised regarding team research. In particular, the perspective is not that of a university administrator (concerned with issues such as the mix of academic ranks in a department, the reward structure, the teaching workload, etc.), or of a government agency (concerned with broad research areas, funding levels, etc.). My focus is thus in contrast with much of the research productivity literature that deals with a more macro-perspective of a complete department, firm or even an entire industry. Often in these studies some form of regression analysis is used linking research productivity to factors such as those listed above (see, for example, Carayol and Matt, 2006; Siegel et al., 2003). There is also a vast literature from a variety of disciplines (economics, sociology, psychology, industrial engineering, and so on) regarding productivity in a general sense as opposed to

research productivity. Illustrative references include Brief (1984), Christopher and Thor (1993), and Buzacott (2002).

A particularly relevant reference is the book by Boice (1990). It focuses on the individual but is restricted to writing which is but one, albeit one of the most important, phases of research. In addition, creativity (or creative problem solving) plays a central role in research and there are many associated references including de Bono (1992), Couger (1995), Evans (1991), Nadler and Hibino (1998), Spitzer and Evans (1999), Vangundy (1987) and von Oech (1998).

The next section of the paper is concerned with the rewards (motivation) of high quality research. Section 3 deals with the myriad sources of research topics. A closely related topic, namely establishing research linkages, is the focus of Section 4. Then Section 5 addresses the choice of a research topic. The generation and retention of ideas, the subjects of Section 6, play a central role in research. A related issue, incubation or the benefits of unconscious thought, is discussed in Section 7. Next, Section 8 addresses the devotion of appropriate amounts of time to research activities. Special attention is given in Section 9 to preparing a research write-up. Then some points related to team research are presented in Section 10. Finally, some miscellaneous points are discussed in Section 11 followed by a brief summary in the last section of the paper.

2. The rewards of high quality research

Why carry out research? There must be some associated rewards and explicitly recognizing these should

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help motivate the individual researcher, hence improve productivity. Possibilities include:

- (i) Self-fulfillment on a job well done (personal accomplishment): You enjoy the process or the product or both. This includes the intellectual challenge of research, the satisfaction of your curiosity and the thrill of discovery.
- (ii) Self-improvement or growth from carrying out the research: This and the previous point are two of the attitudinal variables used by Jones and Preusz (1993) in a study of academic research productivity.
- (iii) Benefits to others: Research results can advance one's department, college, university or academic discipline. In addition, the results of some research projects can certainly impart benefits to organizations, communities, or society in general.
- (iv) Peer recognition both inside and outside the university, which is clearly related to the preceding point.
- (v) Short-term rewards: Some institutions provide financial incentives or course remissions for early publications.
- (vi) Tenure/promotion impact: On a longer term basis high research productivity certainly facilitates the granting of tenure or promotion.
- (vii) Working with and training of students: A significant reward for many researchers is the development of research capabilities of students under their supervision. Moreover, if students do high quality research under the supervision of a faculty member and those students are recognized by receiving best-paper awards, best-thesis prizes, or prestigious jobs, then additional capable students will want to have that faculty member as their supervisor.
- (viii) Funding for further research: Successful completion of research projects obviously increases the chances of additional financial support from granting agencies or on a contract basis.
- (ix) Spinoffs: The first type is a textbook, which was certainly the case for me personally (Silver et al., 1998). The second category, relevant for both the supervisor and students, includes consulting and products (e.g. software).
- (x) Improved teaching: Faculty members are more likely to be familiar with the state of the art if carrying out research. This, in turn, can positively impact the development and delivery of courses, particularly at the more advanced, elective level. More strongly stated by Wilson (2006a): "I assert that high quality research is essential to sustaining acceptable performance as a teacher over the course of an entire career."

3. Sources of research topics

One cannot do research without having a topic! There are many potential sources of topics including:

- (i) Reading the published literature as well as unpublished reports and working papers: Many papers end with a section regarding areas for further research. According to von Oech (1986) "much of creative thinking also involves connecting two previously unconnected ideas and turning them into something new." As Kaplan (2007) notes, the popular and business press and the Internet can also suggest important issues leading to research topics.
- (ii) Talks at local, national and international meetings, as well as professional workshops on specific topics: The presentations, as well as the associated discussions, questions and answers, often suggest new research opportunities.
- (iii) Research information systems (e.g. ProCite (1999)) or search engines (e.g. Google Scholar): These are very useful once you have a general idea of a topic and want to find out who has done what in the area.
- (iv) Refereeing activities: Just as in reading published material one can certainly obtain ideas for potential topics while carrying out referee activities, of course, being careful to not simply plagiarize ideas. Some journals encourage referees to contact authors, particularly once any associated article has been accepted for publication. There are other benefits associated with refereeing. One gets to see the views of other referees as well as the editorial decisions (and supporting arguments) of editors. Moreover, if you decide to subsequently submit a paper to a journal for which you have done a conscientious, refereeing assignment, the editor will at least be familiar with your name and capabilities. Wilson (2002) has much to say on the responsibilities and benefits associated with refereeing.
- (v) Executive teaching and in-house workshops: Throughout my career I have found these to be excellent sources of research topics of interest to practitioners. In fact, in that regard I have proactively sought out suggestions by asking that participants, as part of the registration process, list one or more problems of interest to them. Then, I try to discuss some of these during the teaching or workshop session. Afterwards, I have some potential topics with, in each case, a contact for possible collaboration (for verification of assumptions, provision of relevant data, etc.).
- (vi) Consulting: During a consulting study typically one does not have the time or budget to explore associated research issues in depth.
- (vii) Visits to local organizations: Quite aside from associated research opportunities, these are an excellent source of examples and illustrations for classroom use.
- (viii) Teaching: There are at least two aspects here. First, when making up examples and homework assignments, associated research ideas can be triggered. Second, student projects can be extended or generalized.
- (ix) Colleagues in general: Interactions with colleagues can generate potential topics. In particular, some people are excellent at generating research topics, but not necessarily at completing the associated research. Moreover, as pointed out by Wilson

(2006a), “I have found that good graduate students are often excellent sources of research topics precisely because they have not yet become indoctrinated with the received wisdom of the profession about the boundaries of what is possible.”

The following quotation serves as an appropriate prelude to the ending of this section:

People fail to think of new ideas because they are mentally reactive rather than active. That is, they go through each day unimaginatively, oblivious of problems and issues until someone else solves them, unaware of opportunities until other people transform them into achievements. As Robert P. Crawford observed ‘Luck is often simply a sensing of an opportunity—an opportunity that is there for all of us to see’ (Ruggiero, 1995, p. 87).

The message is to proactively go after research topics! Moreover, it is important to redefine or broaden problems suggested from other sources. By way of illustration, early in my consulting career, I was part of a team that was asked to undertake a major study for an international producer of beer products. The client said that their warehouse was overflowing with inventory and the decision they faced was to decide on the size of a new warehouse to be constructed. Deeper probing revealed that the burgeoning inventory was caused by poor production scheduling. When the latter was properly done, aggregate inventories actually shrunk and sizeable savings in changeover costs were also achieved. Another way of saying all of this is to check the implicit assumptions which are boundaries to one’s thinking. Couger (1995, p. 187) refers to this as “boundary examination”. Elsewhere (Silver, 1992) I have used the expression “changing the givens”.

4. Establishing research linkages

In my experience it is quite important early in one’s research career to establish, and then continue, linkages with other individuals in the same general field. Possible ways of doing this networking include

- (i) exchanging working papers, asking for and providing quick constructive feedback.
- (ii) attending conferences, workshops, etc.—informal interactions, e.g. at coffee breaks, meals, social events, etc., are likely more useful for building research linkages than just attending the presentation sessions.
- (iii) refereeing of papers—some journals identify the referees, at least in a summary fashion. As mentioned earlier, if a paper you have refereed is published, then it can be useful to contact the author(s) regarding ideas, extensions, etc.
- (iv) visiting other facilities (universities, research centres, etc.), both on a national and international basis, including offering to present seminars on research-in-progress as well as completed projects.

Why do these sorts of things? First, they help you stay at the forefront of the field, particularly short-circuiting the long delays until the work of others appears in the open literature. Second, they foster quick feedback on your work. Third, they promote the better visibility of you and your research, which, in turn, can be very helpful in terms of evaluations associated with academic appointments/promotions/granting of tenure.

5. Choosing a research topic

Given a set of possible topics this section addresses how to choose among them. The first concern is whether to do incremental research on an already studied topic or focus on an entirely new research area. Incremental research has a much higher chance of leading to one or more publications in a relatively short time. However, the added value of each such publication will almost certainly be eclipsed by that of an initial publication in a new area. It is probably best to have both types of topics, particularly early in one’s career where completed thesis research is a natural source for incremental further work. In this regard, it is important to strive for at least one good published paper from one’s thesis. However, trying to squeeze as many incremental papers as possible out of a topic may appear attractive in the short run, but can come back to haunt researchers later in their careers. There are individuals in our field who, in my opinion, have hurt their image through adopting this type of strategy.

In the previous paragraph I said “It is probably best to have both types of topics...” More generally, I believe that it is important to develop a portfolio of two or more topics. We will return to this point in Section 7.

But how does one choose a specific topic? There are many factors to consider, among them,

- (i) importance from the perspective of others such as practitioners, leading researchers, granting agencies, society in general, etc.
- (ii) how long is the topic likely to be relevant (to these audiences)?
- (iii) how many people (and precisely who) have been working in the area and for how long?—use of search engines and information systems (mentioned earlier) is helpful here. If highly recognized scholars have spent considerable time on a topic, then the chances are correspondingly low that you will make significant inroads.
- (iv) likely duration of your research on the topic—there are two aspects here. First, the shorter the expected time until tangible output, the better. On the other hand, the area might be fruitful for continuing, meaningful output over an extended period.
- (v) chance of success (in the sense of peer-reviewed publications or other forms of recognition).
- (vi) qualifications/experience of team members which clearly interacts with the previous factor.
- (vii) availability of appropriate data, including possible cooperation of someone in industry.

(viii) your interests—to me this is the most important factor. Work on something that is really of interest to you!

See also the related discussion in Kaplan (2007).

6. Generating and retaining ideas related to research

Of all of the sections in this paper, this one has probably been the most beneficial to my personal research productivity. Here we focus on generating ideas related to research as well as making sure to retain them.

You should immediately jot down ideas as they occur and properly file them (in either paper or computerized form). First, you will get ideas regarding potential topics for research. I have a folder entitled “Miscellaneous Research Ideas” for that purpose. Then, for these potential topics, as well as on-going research projects, you will occasionally obtain ideas. Each topic should have its own folder into which the associated ideas are deposited *without evaluation*. Later, when actively working on the specific topic, the ideas are retrieved, evaluated, modified, combined, expanded, and so on. Shearer (1994) coined the very appropriate term “Idea Bank”. Halmos (1985) in a section entitled “How to Do Research”, has a related discussion of the gradual assembling of notebooks on various research topics.

Writing down and properly storing ideas avoids forgetting or misplacing them. The following quote gives other compelling reasons:

There are two important reasons for writing down your ideas. First, joining mental and physical effort and externalizing ideas often helps clarify them. Second, as any composition teacher will verify, the very act of writing an idea has a way of triggering other ideas (Ruggiero, 1995, p. 125).

As mentioned earlier, creativity plays an important role in research. One of the key principles of creative problem solving is to use divergent thinking in generating ideas on any specific problem area. This should not be restricted to free form methods such as brainstorming (see, for example, Couger (1995) or Vangundy (1987)), but should also use systematic divergence, such as deliberate, logical provocation of lateral thinking (through reversing usual assumptions, exaggeration, cancelling something taken for granted, etc.), as advocated by de Bono (1992).

Another Ruggiero quotation serves to introduce a related point associated with generating ideas, viz.,

Creativity involves a willingness to break away from established patterns and try new directions, but it does not mean being different for the sake of being different or an exercise in self-indulgence. It is as much a mistake to ignore the accumulated knowledge of the past as it is to be limited by it. Being creative means combining knowledge and imagination (Ruggiero, 1995, p. 75).

This argues for being familiar with the relevant literature (again possibly using search engines and

information systems) before getting too deeply into a research topic. In particular, try to avoid reinventing the wheel. On the other hand, be careful to not become constrained to simply following the existing line of research in the topic area.

Both divergent (creative) and convergent (critical) thinking are essential in developing creative solutions to research problems. Without divergence many potential solutions will be missed. Without convergence, the research will not be completed!

One should respect the principles of divergence. The most important (particularly in group research) is to defer discussion, judgement or criticism of ideas generated. Some academics function as if one of their primary responsibilities is to criticize, and not in a particularly constructive fashion, the suggestions of others. de Bono (1986) argues for carefully distinguishing and using different modes of thinking (i.e. not just the critical mode) in problem solving. One should build on the ideas of others. Another principle of divergence is to generate as many ideas as possible (including keeping in mind the earlier discussion about “checking implicit assumptions”). Here’s another relevant excerpt from the literature:

Many of the limits we accept as real are illusory. In a rather simple exercise, two of my colleagues and I instructed one group of subjects to give us as many solutions as they could to a number of ordinary problems (for example: there is no heat, yet you want to stay warm; you want a cold drink but don’t have a bottle opener). After these subjects had run out of solutions, we took the largest number anyone had found and asked another group of subjects to give us that many solutions plus five more. No one in the second group had difficulty meeting this goal (Langer, 1989).

7. The role of incubation

There may indeed be a well-defined time constraint in carrying out a research project. Examples include a time limit on a Ph.D. program or a tenure decision, and a deadline for the submission of a final report on contract research. However, a time constraint can often be self-imposed by a researcher wanting to get the research problem out of the way (for whatever reason), resulting in a poor quality result. As argued in the following, it is best to not rush the research process.

Based on my personal experience (and supported by published material, e.g. Hadamard (1954)), I am convinced that there are enormous benefits of unconscious thought in dealing with research and other types of problem situations. Thus, give yourself time to reflect on problem situations, including deliberately taking significant breaks so as to permit unconscious thought. Results of a survey (Thompson, 1991) reveal that people are most likely to come up with new ideas when their mind is free to wander (e.g. when doing manual labour, when falling asleep/waking up, during a boring meeting,

and so on). Here is a related quote from a Nobel prize winner:

My work is not finished when I leave my workbench in the afternoon. I go on thinking about my problems all the time, and my brain must continue to think about them when I sleep because I wake up, sometimes in the middle of the night, with answers to questions that have been puzzling me.

Albert Szent-Gyorgyi, as cited in [Dement \(1974\)](#).

For years I have kept a paper and pencil near my bed. Without the ability to write down an idea in the middle of the night I either forget it by the morning or manage to remember it but adversely affect my sleeping for the rest of the night! [Florida and Goodnight \(2005\)](#) (note the name of the second author!) make much the same point when they note “It’s more important to capture the innovative insight—whenver it strikes—than to keep rigid work hours.” Incidentally, another way I have found for stimulating new ideas when I am stuck on a problem is to attempt to explain the situation to someone else. This can even be the case when the audience is quite passive.

To allow incubation and unconscious thought you should try to work on more than just a single research topic, i.e. work on at least two separate research problems (the portfolio concept mentioned earlier) or at least one other significant project (e.g. planning a new course, a major vacation, etc.), besides a single research topic. However, do not spread yourself too thin! Having more than one topic helps prevent a tendency of persisting too long when one is stuck on some aspect of research. Here is a proverb (alas—I cannot recall its origin!): “Don’t just keep banging your head against the wall when you can’t find the door!”

8. Devoting time to research

When returning to working on a research subject there is usually a substantial setup time before anything productive occurs. As we know from the fundamentals of production scheduling, a large setup time (or cost) implies the need to devote a large amount of time once past the setup. If not, you spend time setting up, but accomplish very little in the way of progress! Thus, you should dedicate (i.e. set aside and reserve) appreciable, continuous amounts of time to research. Do not try to fit an hour in here and there. In my experience the minimum is several hours, unless there is a very specific, relatively short, task to be completed. In order to promote uninterrupted research time, block out the time on your calendar and try to isolate yourself. Remember that even a very short interruption (such as responding to a question from a partner or professional colleague, a telephone call, an e-mail message, and so on) can lead to a new setup time! One location for possible isolation is at home. In the words of [Waldman \(2006\)](#), in reference to academics, “How many careers exist for which a significant portion of your most productive time can be spent while just wearing your underwear?”

The following quotation relates to the question of when to do research:

I love to do research, I want to do research, I have to do research and I hate to sit down and begin to do research – I always try to put it off as long as I can ([Halmos, 1985, p. 321](#)).

In addition [Boice \(1990\)](#) has noted, faculty members tend to leave writing (and I would add, research in general) to times of the day when they are already tired after having earlier done other less challenging work. This, of course, leads to less than ideal performance in the writing (or research). Thus, it is important to recognize when during the day you are most productive and attempt to devote that time to research activities.

Also be careful to not devote too much consecutive time to research. Without breaks, too long a period tends to decrease productivity, including increasing the frequency of making mistakes. I have found that taking a short break (as little as 15 min) to do some physical activity (e.g. a brisk walk outdoors) improves my overall productivity. [Boice \(1990, p. 66\)](#) makes the same point “Don’t let writing become so fatiguing that you won’t feel like returning soon.”

At the completion of a research session, try to stop at a convenient point in the research (e.g. at the end of a section in a write-up) rather than at a precise point in time. Otherwise significant work-in-progress may be of little value when you next return to the research. This obviously requires some flexibility in your work schedule. If you are at a convenient stopping point earlier than the planned ending time of the session, then it can be helpful to perform filler activities, such as casual reading, that can easily be interrupted without any problem. Incidentally, this last idea was the seed for the Ph.D. research of one of my students, David [Robb \(1992\)](#). Finally, when you stop a research session, mark down the next steps to be undertaken. This can be very helpful when you next return to the research.

Let me introduce the next point with a quotation:

I derived what I call the Coyote approach to solving problems from the popular Warner Brothers’ Looney Tunes cartoons featuring the Coyote in pursuit of the Roadrunner. The Coyote pursues the Roadrunner through a series of comic crises based on his inability to have more than one idea at a time. The Coyote develops that one idea into a plan, which is in turn, implemented without any anticipation or rehearsal of weak points or dangers. The cartoons are funny because they are predictable. Unfortunately, many managers operate in the same way. They may work in a distracting environment which has itself been engineered by continual fire-fighting, producing cyclical crises which are never completely resolved, and where no one ever gets to solve the real problem. The Coyote is very busy. And that’s just the way Coyotes like it ([Newman, 1995, p. 33](#)).

Unlike the coyote, one should try to establish a work plan with associated target dates, which facilitates

blocking out times for research sessions. The contents of the Idea Bank should be helpful in developing an outline of the plan and in roughly estimating the required times for the various steps in the research project. Again, quoting from Boice (who focuses on writing),

Some of the advantages of specifying writing tasks for each session are (a) you'll work with a clear sense of direction, and (b) you'll be able to finish the session knowing that you've done enough for the day. In my experience, writers who work without this task specification often 'dawdle' aimlessly (e.g., they keep reworking an introduction beyond the point of diminishing returns). Equally important, the same writers often feel that unless an entire manuscript is finished, they haven't done enough. No wonder, then, that writing without breaking the task into specifiable units can overwhelm writers (Boice, 1990, pp. 77–78).

Progress against a plan can be quite motivating, but the plan should likely be adapted as you observe your actual progress. Personal rewards (a dinner out with a partner, a day off spent at your favourite recreational activity, etc.), are worth considering. In this vein, Boice (1990) argues for the use of contingency management as follows. Decide which recurrent activities you enjoy and make them contingent on doing valued, but delayable, research tasks.

9. Preparing a research write-up

Some people are very good at developing research ideas but fail in the crucial stage of completing a write-up suitable for publication and dissemination. The book by Boice (1990) contains many helpful suggestions regarding effective and efficient writing (e.g. guidelines for rearranging the writing surface and its environment, changing writing habits, improving drafts of manuscripts, etc.). Some of these are blended into the discussion in this section. Another, very useful reference is Wilson (2006b). Boice also provides an annotated bibliography of some 100 references on writer's block and other hindrances to writing. Incidentally the following website is devoted to writer's block: <http://www.writersblock.com/>.

There are some individuals who strive for perfection in their research before writing it up. Here's a related quotation:

If you spend too much time warming up, you'll miss the race. If you don't warm up at all, you may not finish the race.

Grant Heidrich, Runner (as cited by von Oech, 1986, p. 109).

If you spend too much time and effort on very incremental changes/improvements in the research, you will either never finish the write-up or it will be superseded by something being published by someone else! On the other hand, if you do not do enough preparatory work, you will not be able to prepare a meaningful write-up.

Related to the above point some individuals, particularly those early in their careers, tend to try to put too much into a single paper. However, let me quickly stress

that I am not advocating attempting to maximize the number of publications on a single topic by splitting it into many modules!

You are very unlikely to be able to prepare a draft of a complete paper in a single sitting. I have found that, on average, I can draft something on the order of five to seven typed, double-spaced pages. When you next return to the writing you should reread and edit what you did on at least the last occasion (which was probably a minimum of a day earlier). This also facilitates a smooth transition into the new material.

With regard to the perfection issue, Boice (1990, p. 44) argues for sharing imperfect written copy with colleagues, rather than only finished documents as there is a natural tendency to be more resistant to criticism of the latter. The risk is that repeated requests for such evaluations may overload colleagues and also significantly delay the ultimate publication. My own preference is to prepare a paper in, what I initially consider to be, a form suitable for submission to a target journal. I send this version to a few professional colleagues, partly to make them aware of what I have accomplished without having them wait through the long delay until possible subsequent publication. In addition, I seek input from them, but I do not wait too long for such evaluations before making the (possibly adjusted by feedback) submission. If the referees request a revision, relevant input from other sources, received after the submission date, can be added when responding to the reviews. Making constructive changes, beyond what reviewers have requested, is generally viewed very favourably by those individuals. Speaking of referees, do *not* count on them to edit your work, i.e. do not be negligent in your preparation (including making sure to follow the guidelines provided by the journal targeted). Why unnecessarily alienate these people who represent a possible barrier to the publication of your work? Consider the use of a copy-editor or relevant reference books (such as Huckin and Olsen (2001) or Fowler and Aaron (2004)) particularly if you are not writing in your native language. Some journals provide a set of questions to be answered in a referee's report. These or a generic list provided by Wilson (2002) should be taken into account in the preparation of a manuscript.

Prior to beginning a write-up I have found it extremely helpful to prepare an outline of the document, annotating it with indications of where all of the miscellaneous ideas (that I have been collecting and filing in the Idea Bank) will be placed. Incidentally, the same idea can be relevant to more than one section of the paper. Before doing this I go through all the points in the Idea Bank giving each a number code on a summary sheet. Fig. 1 is a typed illustration of a portion (just 2 out of the more than 20 sections and subsections) of the outline that I used (in a handwritten form) for a recently published major paper (Silver, 2004) for which I had accumulated over 100 points, many divided into numerous subpoints, in the Idea Bank! Then in the actual writing of any particular section the listed points are fleshed out. The preparation of the outline and the placement of the various points obviously takes quite a bit of time. However, I believe that it is a worthwhile investment that pays big dividends when you

1. Introduction

- Before drafting this, also go over points for next section.

Includes definition, pt. 12c, 15a, 27, 27b, 34c, 37c, reference major books and articles on heuristics, 42b, 64 (1st point only), 97, 94, 77b.

2. Why Use a Heuristic Method?

NP complete/hard, 13a, 15a, 15c, 25a, 83a, 83i, pt. 41, 7b, 12d, 13bi, 13biii, 26g, 27a, 31a, 31b, 32a, 32b, 34a, 34b, 37b, 39, 42a, 42c, 45c, 53c, 54, 57a, 59, 91, 98, 67c, 67s, 69, 78, 82a, 82b, 82ii, 90c, 90d, 90e, 90f, 101.

Fig. 1. Part of the outline of the heuristics paper.

do the actual writing. Another helpful reference on the development of manuscript outlines is [Matthews et al. \(2000\)](#).

In writing up their findings there is a tendency for researchers to quickly move to a detailed description of the methodology used and the associated findings. However, it is crucial to first carefully motivate the importance of the problem being studied and/or the results obtained. In other words, in the introduction to the paper it is essential to articulate what you have studied and why there was a need for such research ([Waldman, 2006](#)).

The appropriate use of tables and figures can play a crucial role in properly conveying information to the reader. An excellent associated book is [Tufte \(2001\)](#).

Referees were mentioned above. Here are some suggestions for responding to their critiques. Constructively defend the important issues where you believe that your viewpoint is more appropriate than that of the reviewer. However, be flexible on more minor issues. Also, carefully word your responses, i.e. do not needlessly alienate the referees or editor. As [Boice \(1990, p. 102\)](#) says, "Try to respond to your critic with calm agreement. You can almost always find something in the criticism that is conceivably correct, at least from the critic's perspective."

10. Team research

This section addresses the common situation of conducting research in a team, including faculty with students. The first point concerns choosing co-workers. I have learned through experience that you should be as concerned about the work styles as you are about the subject knowledge and skills of your fellow researchers. Conflicting work styles (e.g. with respect to sticking to agreed assumptions or notation, informing co-workers of progress, etc.), can lead to much frustration. Team members having similar capabilities in terms of subject knowledge and skills may make it easier to work together. However, divergence of views may produce innovative results and may be more fruitful for both parties. An example of the latter is where one person is excellent at generating general ideas and another is effective in filling out the details.

1. Introduction

- Before drafting this, also go over points for next section.

Includes definition, points 12c, 15a, 27, 27b, 34c, 37c, reference to major books and articles on heuristics, 42b, 64 (first point only), 97, 94, 77b.

2. Why use a heuristic method?

NP complete/hard, 13a, 15a, 15c, 25a, 83a, 83i, pt. 41, 7b, 12d, 13bi, 13biii, 26g, 27a, 31a, 31b, 32a, 32b, 34a, 34b, 37b, 39, 42a, 42c, 45c, 53c, 54, 57a, 59, 91, 98, 67c, 67s, 69, 78, 82a, 82b, 82ii, 90c, 90d, 90e, 90f, 101.

Remote collaboration has become much easier due to the communication capabilities of e-mail, text messaging, faxing, etc. Thus, it is no longer critical for co-researchers to be physically located at the same institution. I have been fortunate to be able to fruitfully collaborate with people located in several countries, even including having co-authored papers published where I have never physically met with my co-author! Nevertheless, the following quotation indicates that an important ingredient of divergent thinking may be lost in remote communications:

My experience with collaborating on problems with people face to face and electronically makes me convinced there is a difference in what happens. Sitting in a room with someone discussing a problem is full of half finished sentences, long pauses, but you learn how the other people really see the problem. Electronic communication by definition means the messages are larger and more thought out. You only communicate what you think might work, not your first reaction which you decided was wrong, but may spark something in your collaborator ([Thomas, 1995](#)).

The next point relates to the follow-up to a team research meeting. I strongly recommend that one person present be responsible (and know that before the meeting starts) to prepare a written summary of key points discussed during the meeting. This write-up should be given to the others present within a short interval after the meeting, certainly no more than 48 hours. There are at least three associated benefits. First, the writer is forced to immediately go over and understand his/her notes. Second, it provides the others a chance to quickly query something that they do not understand or with which they disagree. Third, it is very helpful to read the write-up just before the next group meeting.

The previous section was devoted to preparing a research write-up (not just associated with a single meeting, but rather a document being prepared for publication). There are some special concerns when that

document emanates from team research. Early in my career I learned the hard way how *not* to prepare such a document. My co-researcher and I literally tried to jointly compose a paper sentence by sentence. What a frustrating experience for both of us! By trial and error, I have found that the following process works well. First, jointly develop an outline (again likely making use of the points in the Idea Bank). Then, either one person does the entire draft, or specific sections are assigned to different authors. In the latter case a single individual should be responsible for doing a final editing to ensure consistent style, proper cross-referencing, etc. However, prior to this step, each team member should review and constructively critique what the others have drafted, possibly leading to rewrites, additional reviews, and so on. More fundamentally there are the issues of who should be listed as authors and in what order. Wilson (2002) provides some helpful principles of authorship.

11. Some miscellaneous ideas

This section includes a number of ideas which I felt did not conveniently fit in earlier portions of the paper. The first four relate to generating ideas for research opportunities (i.e. topics) and for creatively dealing with a given topic. They are adapted from von Oech (1998) who lists (more than four) mental locks inhibiting creative thinking. I prefer to slightly change the wording to “mental blocks”.

- (i) Mental Block 1: There is a single right answer—in reality most problems have many right answers depending upon what you are searching for, particularly if you permit redefining the problem (e.g. by changing some of the implicit assumptions). If researchers think that there is only one right answer, then they stop looking as soon as they have found one. Instead, look for at least a second right answer.
- (ii) Mental Block 2: Humour and research (or work in general) do not mix—in reality, humour can be very effective in aiding creativity, communication and productivity in research and other types of work. The “coyote” analogy, used earlier in Section 8, is an illustration of humour employed to convey an important, underlying message.
- (iii) Mental Block 3: To err is wrong—most of us have been indoctrinated since very early in life to think this way. Thus we try to avoid situations that can lead to error making. Moreover, whenever an error occurs, the usual reaction is “Oh no! What went wrong this time?” Instead it is far better to ask “How can this unexpected result lead to a better understanding of the subject, or, more generally, how can I seize on this opportunity?” In other words, mistakes should be viewed as learning experiences. There are many practical examples of inventions that originated from errors (or unanticipated results) in earlier experimentation. These include penicillin, velcro and skateboards. In my own experience in mathematical modelling, unexpected (bizarre) results often provide

insights regarding the behaviour of processes or systems under study. An example of this type of insight is described in Silver (1973).

- (iv) Mental Block 4: That’s not my area—some of the best ideas come from looking outside of your field. Thus, try to make a point of reading general interest articles and interacting with individuals from other disciplines. A practical illustration is provided by von Hippel et al. (1999) who describe a breakthrough by a team at 3M in the area of surgical drapes (the material that prevents the spread of infection during surgery). The breakthrough was a result of ideas from two surprising sources, namely veterinary hospitals and Hollywood makeup artists!
- (v) Early on you should attempt to identify what you are attempting to accomplish in the research. Another way of saying this is that it is very helpful to formulate one or more hypotheses that you plan to prove or disprove in your research.
- (vi) Be careful about discarding research material—you never know when at least parts of earlier, dead-end, material may be useful. For each research topic I have what I call a “non-current” file for retention purposes.
- (vii) Don’t go overboard in collecting information or data—here is an associated quote:

The indiscriminate amassing of information inherent in ‘finding out all there is to know’ not only wastes time, effort, and money, it can actually impede solution of a problem by burying you under an avalanche of irrelevant, unmanageable detail. In short, it causes, “analysis-paralysis (Nadler and Hibino, 1998, p. 233).

You should identify the purpose of any information or data before you collect it. Some researchers continue to collect information, primarily as a stalling mechanism to avoid what they perceive as the much more difficult phases of hypothesis building, synthesis and write-up.

- (viii) Avoid the tendency to always use the same tool/method to solve all problems—Bowen, in an article that also deals with properly formulating (research) problems, points this out in a colourful fashion as follows:

...the story of the man with a screwdriver. In idle moments, he wandered around his house looking for loose screws. Eventually, reaching the point of no return, he decided to deal with protruding nails. He used a file to make a slot in these and then...well, he was a screwdriver man! (Bowen, 1998).

- (ix) Be organized in other activities—being organized in other activities (such as course design and delivery) benefits research productivity by reducing the amount of fatigue from these other activities and also by making more time available for research. One of my published articles (Silver, 2001) should be helpful in this regard.

12. Summary

In this paper, I have presented a number of ideas and approaches that I and others have found to be helpful in enhancing research productivity. Very few of these were known to me when I commenced my academic career. By writing this article I am hopeful of shortening the associated learning curve for readers, particularly those early in their research careers. Perhaps an overriding theme of the ideas and concepts, that I have found particularly helpful, is to take a broad, rather than a narrow, perspective in identifying research topics, carrying out research and writing up the results. Probably the most useful tool, from my perspective, has been the Idea Bank discussed in Section 6.

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